existing collection of North American antiquities, I can safely assert that they are totally abnormal in character, that is, unlike any pre-Columbian stone carvings thus far found in the United States. They neither show the characteristics of the stone sculptures discovered in mounds, nor do they resemble the well-known specimens of modern Indian art. In short, they are not typical at all, unless, indeed, we deem them sufficiently important to form a type for themselves. Such an importance, however, I cannot concede to them, believing that they originated in comparatively modern, certainly in post-Columbian, times, and were made by a few individuals of the Indian, or, perhaps, even of the Caucasian, race. The rude attempts at imitating animals of the Old World are conclusive evidences that the makers either had seen such animals, or knew at least that they existed.

The carvings, it should further be taken into account, are executed in soft potstone, a material easily yielding to the effects of exposure, and hence a short lapse of time would have sufficed to give them the appearance of real antiquities. In fine, I consider these carvings as a modern intrusion, and would deem it an extremely hazardous attempt to make them the basis for speculations bearing on the ethnology of North America.

CHARLES RAU,
Curator Department of Antiquities,
U.S. National Museum
Smithsonian Institution, Washington, D.C., June 27

The Influence of Light on the Development of Bacteria

IN NATURE for July 12, 1877, there appeared a short communication from Messrs. Downes and Blunt summarising the conclusions at which they had arrived as the result of investigations on the influence of light on the development of lower organisms. The experiments were described in detail in the *Proceedings* of the Royal Society for 1877 (vol. xxvi. p. 488), and were considered by them to show that light is inimical to the development of bacteria in Pasteur's solution; but that for the full effect direct insolation is needed. Exposure to the sun's rays, according to them, may simply retard development, or it may completely sterilise the solution, by killing bacteria or their germs contained in it. In a second paper read before the Royal Society (*Proceedings*, vol. xxviii. p. 199), some further experiments were detailed, which, however, did not, I venture to think, do much towards settling the difficulties of the question. In the same volume of the *Proceedings* (p. 212) Prof. Tyndall supplied observations of his own, which confirmed the conclusions of Messrs. Downes and Blunt, in so far as the retardation of development was concerned, but differed on the point of sterilisation by bacterial destruction being attainable by insolation. At the last meeting of the British Association, Prof. Tyndall returned to the subject (NATURE, September 15, 1881), and related some further experiments confirming what he had previously stated.

I have recently made a considerable series of experiments, with the hope that under our bright Australian sun I might get results decisive on the point of difference between these inquirers and also confirming or negativing the result on which they were agreed. I made use of Cohn's solution as the cultivation fluid and common one-ounce phials as the vessels. The solution was inoculated with a small quantity of fluid swarming with bacteria (*B. termo*), the bottles plugged with cotton wadding and exposed fully to the sun. Some of the experiments were made in the hottest weather of February and March, and the later in April. To begin with, I simply placed the bottles on the outside of a window-sill, on which the sun shone during the greater part of the day, a temperature of 124° F. being noted on one occasion, and that probably not the highest reached. My first results seemed fully to confirm the conclusions of Messrs. Downes and Blunt, complete sterilisation apparently being sometimes attained, at least as far as bacterial growth was concerned, the destruction of mould spores, as also noted by them, not being so easily accomplished. Suspecting at last that the effect might possibly be due as much to elevation of temperature as to any special chemical or other action of the sun's rays, I varied my procedure. I was led to do this in part by the circumstance that I had not seen diffused light check in any way the bacterial growth, when the solution exposed to it was kept at the same temperature approximately as that in other bottles closely wrapped in brown paper. It did happen, indeed, that the exposed solution became opalescent *sooner* than that which had been guarded. The method ultimately adopted was, to suspend the bottle outside of a window, and the particulars of one experiment will make clear what resulted, and show the general method. On April 6, at 2 p.m., the weather being bright but cool, these bottles, containing each two drachms of inoculated solution, were suspended outside of a window. The 7th was cloudy, the 8th bright and cool; and on the 9th, which was bright and warm, all were still found transparent, and, at 9 a.m., one was brought in out of the sun. On the 10th, which was also bright, another was taken in at 9 a.m., the one which was left out showing, at that time, faint signs of cloudiness. A thermometer hung up beside it marked a temperature of 98° F. Next day, the 11th, at 9 a.m., the exposed solution was quite milky, the others just beginning to show traces of opalescence, the one removed on the 9th being least advanced. This experiment, even by itself, was almost decisive. It established the fact that insolation by itself does not prevent the growth of bacteria in a perfectly transparent medium, and does not even retard it, relatively to the time needed in solution less exposed, but kept at a rather lower temperature. In another experiment I found two bottles continuously exposed to the sun during two bright days, become milky, the bacterial growth, in fact, only beginning then, no trace of cloudiness having shown itself during five previous days which were dark and cold.

The conclusion I came to of necessity was, that the bacterial development was mainly, if not wholly, dependent on temperature. On referring to a paper by Dr. Ed. Eidam, in Cohn's *Beiträge zur Biologie der Pflanzen*, Heft iii., I found that he had proved that the *bacterium termo* passes into a torpid condition (Wärmestarre), when exposed to a temperature of between 40° and 45° C. (104°–113° F.); is killed by seven days exposure at 45° C., by fourteen hours at 47° C. (116°·3 F.), by three to four hours at 50°–52° C. (122°–125·6° F.), and by one hour at 60° C. (140° F.). In this country there need be no difficulty in getting a heat in the sun, greater than even the highest of these; and I should think it possible enough in England, on a hot summer day, to get a temperature in exposed situations considerably over 104° F., sufficient to paralyse the bacteria, or even to kill them, if the exposure was long enough continued. Any explanation of the difference between the results of Messrs. Downes and Blunt, and those of Prof. Tyndall, on the point of complete sterilisation, is simply that, while they used small test-tubes, his solutions were contained in flasks, and that the larger body of fluid less easily reached the highest temperature attainable under the conditions. The different results, with very small and larger tubes, observed by the former, and to them evidently inexplicable, if not simply accidental, is best explained on the same principle.

It is true that Prof. Tyndall agrees with the other inquirers in disclaiming the notion that the apparently inimical influence of light can be ascribed to difference of temperature; but it is evident that it had not occurred to them, that any possible elevation of temperature could act otherwise than by favouring bacterial growth and multiplication. This is plain, from the words used by Messrs. Downes and Blunt (*Proceedings*, vol. xxvi. p. 491), when, trying to account for some anomaly, they remark that "external conditions—notably temperature—may retard or counteract the preservative quality of the solar rays. It must be understood, however, that the putrefactive tendency of warmth does not, in our experience, with this solution at least, override what we termed the preservative quality of light; for, provided that there was the full amount of sunlight, we have preserved tubes exposed *continuously* from day to day as readily in hot weather as in cool." Prof. Tyndall, in his recent paper, speaks of his flasks having been exposed to strong sunshine for a whole summer's day; and, with reference to the more rapid occurrence of turbidity in those which had been shaded, adds: "This result is not due to mere difference of temperature between the infusions. On many occasions the temperature of the exposed flasks was far more favourable to the development of life than that of the shaded ones."

I feel the boldness of criticising the conclusions aimed at by such a famous investigator as Tyndall, and all the more when these are in accord, in the main, with those of other inquirers, the joint results having hitherto, to all appearance, been accepted as unimpeachable. I do so only after careful observation and consideration, and with the hope that further investigations, made with due precautions, will establish the correctness of what I have here stated. My researches in detail will be brought before the Royal Society of Victoria at the next meeting, and I will take the liberty of forwarding you a copy of the paper when

printed, and of distributing a few others. It contains an account of experiments for the purpose of testing the action of sun and air on dried bacteria, which have some interest, but which the fear of trespassing unduly on your space prevents me from entering on here. JAMES JAMIESON

Melbourne, May 22

Fireballs observed in the Netherlands

IN the well-known "Meteoric Astronomy" of Dr. D. Kirkman, p. 67, is to be found the following note, on the occasion of the interesting shower of dust and aërolites in Calabria on the 13th and 14th of March, 1813. "The date of this remarkable occurrence is worthy of note, as a probable aërolite epoch. From the 12th to the 15th of March we have the following falls of meteoric stones. . . ." &c. (7 cases).

In reference to this note it may interest your readers that on the night of 12-13 March last two great meteors were observed in two different places in the Netherlands. The first observation, made near the village of Haren (four and a half kilom. S. of Groningen), by the schoolmaster, Mr. H. Bos, at 1 a.m., refers to a bright fireball, shining with a splendid "bluish red" light, illuminating the night, leaving a violet train, which lasted some moments. The path seems to have been from a point not far beneath the zenith, in an azimuth of 115° to another at 108° azimuth, and had a length of 45°, which was traversed by the phenomenon in 4 or 5 seconds. After 85 seconds—measured afterwards by means of a watch and by the distance of the objects which the observer had passed, going with a known velocity—a full detonation, like a distant cannon-shot, was heard in the same direction.

On the same night, and at the same hour, another fireball was seen near the village of Bergen, in North Holland. The schoolmaster, J. Francken, gives me the following indications of its path, found by him after having interviewed the observer. It went from N.E. in altitude of 50° to S.E. in an altitude of a little less than 40° It is therefore impossible that this phenomenon should be the same as the former, the direction of the course being opposite. A second observer gave nearly the same direction.

It is worthy of remark that another violet meteor had been seen near Haren on March 12 at 8 p.m. in the S.W.[1]

A fourth meteor of the greatest size, described as being as great as the full moon, was seen by three policemen, from whom I have received tolerably harmonising records, though they were standing in different positions in the town. The time of appearance was May 1, at 4 a.m. precisely, or perhaps three minutes afterwards, and the direction of the course was S.S.E. to N.N.E. It was described by one of the observers as beginning like a shooting star (though already lightening the sky), falling downwards and rising again in a curve from S.S.E. to N.N.E., increasing in the meanwhile to a great ball of a splendid purple light, and showing a train of a silvery colour. The phenomenon lasted 50 seconds (?) measured by a watch. No sound was heard. The disappearance was instantaneous. It is uncertain if an explosion was really observed. The altitude seems to have been at the beginning, and at the end point perhaps 10°, somewhat higher in the middle, as I have attempted to determine *in loco*. The second observer estimated the duration of the phenomenon to be 13 seconds. Even when this is accepted, the body must have been very distant, and of a great volume, though increased apparently by irradiation.

The same morning, at 3h. 45m. a.m. there was also seen a great meteor, going from W. to E. near Enumatil (8 kilom. W.), and at Assen (S. from Groningen). It seems not to be identical with the former. At Assen there was heard a buzzing sound. The Enumatil observers compare the phenomenon, whose colour was white with a red train, to a drum-major's staff. The Assen observers speak of a bluish train or tail, which seemed to be smoke. The same was seen by the sluice-keeper, G. Mulder, at Veenhuizen (W. of Assen), who heard also the buzzing sound, and gives also the direction, W.—E. The ball passed S. of the zenith (Assen).

Still another great fireball was observed at Bourtange (S.E. of Groningen) at 5h. 12m. (local time) a.m. on the same morning. It had a quick motion from S.W. to E. It gave the impression of being very near the earth, only some meters above the houses. It seems to me uncertain whether it was a globulous lightning or a true fireball. The phenomenon showed a fiery tail and exploded without any sound.

Recapitulating, there seems to have been observed the following fireballs of great size:—

March 12, 8 p.m. (Groningen M.T.) at Haren.
,, 13, 1 a.m. ,, ,, ,,
,, 13, ,, ,, Bergen (N. Holland).
May 1, 3.45 ,, ,, ,, Enumatil.
,, ,, ,, ,, Assen } Probably
,, ,, ,, ,, Veenhuizen } the same.
,, 4, ,, ,, ,, Groningen.

Finally a fireball or a globulous lightning.
May 1, 3 45 a.m. (Groningen M.T.) at Bourtange.

Groningen, June 19 H. J. H. GRONEMAN

Aluminium for Movable Coils

AT the Oxford meeting of the Physical Society, after Dr. W. H. Stone's interesting description of an electrodynamometer designed for medical purposes with the movable coil, made of aluminium wire for the sake of lightness, I took the liberty to remark that about eight years ago Dr. Werner Siemens had made use of aluminium wire for the movable bobbin of his *dynamo-relay*.

I was then under the impression that this fact was probably not known in this country; a friend has, however, since called my attention to a short paragraph in the *Telegraphic Journal* of 1878, p. 53, in which it is already mentioned.

One of these dynamo-relays was shown working at the Paris Electrical Exhibition, and Messrs. Siemens and Halske have made use of the same principle in their so-called *soot-recorder* (see *Telegraphic Journal*, 1878, p. 90), an instrument well suited for the registration of currents of varying direction and strength.

At the meeting I further said that in some of the coils made of very thin aluminium wire, I had found an increase of resistance after the lapse of some time, and that this increase was proved to exist at the place where two lengths of wire had been joined by twisting them round each other.

Some experiments were afterwards made to coat the ends of the wire with an electrolytic deposit of copper, and then solder them together; but the best and most natural way to overcome the difficulty is to make the coil all of one length of wire, and thereby dispense with all internal joints.

A similar increase of resistance at the place of contact between aluminium wire and mercury I had already observed several years previously; the cause of it seems to be the formation of a very thin film of aluminium oxide on the surface of the wire.

I have been led to make the above remarks after perusing the closing paragraph of Dr. Stone's article in NATURE, vol. xxvi. p. 201. EUGENE OBACH

Woolwich, July 3

The Recent Weather

THE article published in NATURE, vol. xxv. p. 225, entitled "Recent Weather," attracted attention from meteorologists in China from the fact that the extraordinary character of the season therein discussed was observable in China also In my Report on the Health of Wenchau, I referred to the unprecedentedly high reading of the barometer in this part of the world, at the same time that a like phenomenon was observed in Western Europe.

NATURE records that November last showed the highest thermometrical range that has been known since thermometers came into use. On referring to the tables of Dr. Zrightsche (director of the Belgian Observatory at Peking), I find that the mean temperature at Shanghai of that month for a period of twelve years falls considerably below that of the record for November last; and finally we learn from NATURE that the winter of 1881-82 in Western Europe was an "open one," which was the case in Eastern Asia; the port of Tientsin, for example, having been closed by ice later, and opened to navigation earlier than usual. When the meteorological reports are all gathered in, it will be found that the abnormalities which characterised last winter were coincident with like phenomena in this part of the world. D. J. MACGOWAN

Wenchau, May 17

[1] In the German journal, *Sirius*, edited by Dr. H. J. Klein, Bd. x. (1882), p. 40, is mentioned likewise a fireball of the full moon size, from March 13, 1875, by Mr. T. Köhl.